need for improvement in this matter. It is, of course, impossible for Government to interfere in squabbles about disputed boundaries, &c., and hence jealousy and stubbornness will continue to put considerable areas of coal beyond the chance of being "won." But surely something might be done towards increasing the number of our mining schools; and, as Prof. Hull suggests, the Legislature might establish some educational test without which no one should be allowed to have the supervision of colliery workings. With well-educated managers the waste of coal arising from ignorant methods of working would be checked, and we should hear less frequently of those frightful accidents which ever and anon throw whole mining communities into mourning. J. G.

The Natural History of Plants. By H. Baillon; translated by M. M. Hartog. Vol II. (London: L. Reeve and Co., 1872.)

PURSUING the somewhat erratic arrangement to which we alluded in our notice of the first volume of this work (see NATURE, vol. iv. p. 199), Prof. Baillon proceeds to an account of the small order of Connaraceæ, the three sections of the large order Leguminosæ, viz., Mimoseæ, Cæsalpinieæ, and Papilionaceæ, and then goes off at a tangent to four orders of Incompletæ, viz., Proteaceæ, Lauraceæ, Elæagnaceæ, and Myristacaceæ. The same plan is pursued as in the first volume, of giving first of all a general sketch of the characters of the order, and then dividing it into a number of "series," each containing one or more genera. An immense mass of information is thus collected, though wanting in convenient arrangement. The references to original authorities are, however, commendably copious. The illustrations, as before, are excellent, the translation apparently well and carefully done.

Memorandum des Travaux de Botanique et de Physiologie végétale qui ont été publiés par l'Académie Royale de Belgique pendant le premier siècle de son existence, 1772—1871. Rapport Séculaire per E. Morren. (Bruxelles: Hayez, 1872.)

THOUGH Belgium has not produced any botanical star of the first magnitude, yet a large amount of excellent work has been done in the little kingdom, especially during the period of its independent existence, since 1830, as shown by the labours of Decaisne in the flora of Japan, and of Galeotti in that of Mexico, of Jean Kickx in cryptogamic botany, of Charles Morren in teratology and general morphology, and of Quetelet in the periodic phenomena of vegetation. M. E. Morren's short abstract of the service done by his countrymen in each department of botany, with a list of the dates of publication of the various memoirs, is a useful contribution to the history of the science.

Results of Five Years' Meteorological Observations for Hobart Town.

WE are here presented with carefully constructed tables and valuable remarks on the climate and vital statistics of Tasmania. It includes the results of observations for the five years ending in 1870, with which are incorporated the statistics for the previous twenty-five years, so that it presents us with a complete set of statistics of meteorology for thirty years. That the observations have been carefully and correctly made is proved by the fact that the results of thirty years' observations agree very closely with those of the twenty-five years, the difference in many cases amounting to only a second or third place of decimals. The observations for the five years ending 1870, have been made gratuitously at the Toronto Observatory, by Mr. Francis Abbott, the tables having been constructed by Mr. T. Roblin, Curator of the Museum, and revised by Dr. E. Swarbeck Hall, who appends an elaborate and carefully drawn up health report for Tasmania. The introduction, among other matters, contains a descriptive list of the various instruments used; the set seems to be complete, and all the apparatus trustworthy. The following are the mean resultants from the thirty years' observations for Hobart Town from 1841 to 1870 inclusive:—Barometer (at temperature 32°), 29·580. Thermometer, mean temperature, 54·72; mean diurnal range, 17·91; mean solar intensity, 93·39; mean terrestrial radiation, 43·01. Humidity of air—Dew point—mean position, 45·49; humidity of air, ·75; elastic force of vapour, ·316; condensation, rain in inches, 1·89; number of days on which rain fell, 11·66; Ozone, mean daily amount, 7·24; prevailing direction of wind, N.W., S.E.; prevailing force of wind, 58·37 lbs. per square foot.

Jahrbuch der k. k. Geologischen Reichsanstalt, xxii. band, No. 2. (Wien.)

FRANZ RITTER V. HAUER gives, in this number of the Year-book, an outline sketch of the sedimentary formations met with in Austria. He arranges his materials somewhat after the manner of Studer's "Index der Petrographie," the names of the various deposits following in alphabetical order. The geological horizons are briefly indicated, and copious references to authorities are given. The descriptions are necessarily brief, but they are clear and comprehensive, and the paper will be invaluable to those who may desire to widen their acquaintance with Austrian geology. Among the mineralogical contributions, we notice the description of a new mineral from Mexico—*Guadalcazerite,* the composition of which is given as $6\ H_g\ S + Z_n\ S$. From the laboratory of Prof. Bauer come several useful rock-analyses, chiefly ironstones. Professor Tschermak gives a description of sundry meteorites, and some account of basalts, metaphyres, and other eruptive rocks from the Caucasus. There is also an interesting paper by Professor Inostranzeff on the Vesuvian lavas of 1871 and 1872.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Prof. Balfour Stewart on the Spectroscope

I BEG to say a few words in reply to some statements in the letter of Prof. Balfour Stewart in NATURE, February 20.

1st. I wish to state that I had no knowledge of Mr. Proctor's letter in the *English Mechanic* until I saw it by accident in a copy of that journal which had been sent to me for another matter. I have never made the claim to which Prof. Stewart refers.

2nd. The note in Schellen's "Spectrum Analysis," of which Prof. Stewart asks an explanation, consists of three statements of fact.

α "Though to Mr. Lockyer is due the first publication of the idea of the possibility of applying the spectroscope to observe the red flames in sunshine, as a matter of history it should not be passed over that about the same time the same idea occurred independently to Mr. Stone, of Greenwich, and to Mr. Huggins." I wish to remark that I made no claim on Mr. Stone's part or on my own. On the contrary I said expressly "*as a matter of history*" these facts should not be passed over. I conceive that this statement of facts ought to have a place in a book which professes to give the history of the subject.

β "These observers were, however, unsuccessful in numerous attempts which they made to see the spectra of the prominences, for the reason probably that the spectroscopes they employed were not of sufficient dispersive power to make the bright lines of the solar flames easily visible." Prof. Stewart remarks that he "cannot yet understand" why I failed. The reasons can now be a matter of conjecture only. All workers in science know how much more difficult it is to discover the unknown than to recognise the known. It may be that I passed over too carelessly the deep red (C) and the blue (F) where the brightest lines occur. The observation of the bright lines in the star in Corona B (May 1866), in which the same sifting principle of the prism came into operation, should have suggested to me to look for the lines of

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hyrdogen. Later (June 1867) Mr. Johnstone Stoney showed, in an important paper on the "Constitution of the Sun and Stars," that hydrogen was to be expected. I acknowledge that I ought to have found the lines.

γ "When the position of the lines was known, Huggins saw them instantly with the same spectroscope which he had previously used in vain." Of course in this remark I refer only to my own experience; I do not wish to be thought to imply that such assistance was either needed or received in the case of any other observer. Prof. Stewart asks why I did not see the lines sooner after receiving the news from India. This question awakens painful memories. At the time the Indian reports reached me I was watching by the bed of the dying, and a few days afterwards I suffered so severe a bereavement that I was unable to resume work in the observatory until the beginning of December, when I saw the lines. WILLIAM HUGGINS

The Beginnings of Life

IT seems advisable for me not to pass without comment the communication made by Dr. Wm. Roberts, of Manchester, in last week's NATURE.

Dr. Roberts calls attention to what he considers two possible sources of error in my experiments. The first is the "possibility of the introduction of atmospheric germs at the moment of sealing the vessels," owing, as he says, to the fact "that the sealing can only take place just as ebullition is about to cease," and to the consequent risk of some "reflux of air into the flask." After Dr. Roberts has made a series of experiments similar to those which have been recently cited in these columns (see NATURE, Feb. 6, p. 275 with *Erratum* in Feb. 13, p. 296), he may perhaps be a little less apprehensive as to this source of contamination. It is, however, not the fact that flasks cannot be sealed during ebullition, and this I shall be very happy to demonstrate to Dr. Roberts. Moreover, if he will refer to Dr. Sanderson's letter, Dr. Roberts will find that in speaking of the sealing of the flasks in the blow-pipe flame, he says care was taken "to continue the ebullition to the last." And in several series of experiments M. Pasteur also made use of flasks which had been sealed during ebullition—believing that in so doing he was experimenting with vessels from which all living germs had been excluded. Speaking of the preparation of such flasks, Pasteur says (*Ann. de Chim. et de Physique*, 1862, p. 74): "je ferme l'extremité effilée pendant l'ebullition. Le vide se trouve fait dans les ballons." No one has hitherto questioned M. Pasteur's skill as an experimenter.

The second alleged source of error is, according to Dr. Roberts, much more important. My mode of experimentation, he says, "does not insure that the entire contents of the flask are effectively exposed to the boiling heat." Although Dr. Roberts confirms my statement that many fluids treated in the manner I have described do soon swarm with living things, he seems to think their appearance may be due to the fact that several of the mixtures "froth excessively in boiling, and spurt about particles which adhere to the glass, and probably some of these escape the full effect of the heat." I feel quite sure that in my experiments no portion of the inner surface of the glass has escaped the scathing action of the boiling fluid The vessel has generally been more than three-fourths full before the process of heating has been commenced, so that when ebullition occurs the fluid has always swept over the previously uncovered inner surface and, as Dr. Sanderson testifies, "during the boiling some of the liquid was frequently ejected from the almost capillary orifice of the retort." The inner surface of the vessel was, in fact, always thoroughly and repeatedly washed with the boiling fluid, nearly half of which has been spurted away in order that I might effect this object

Dr. Roberts says:—"Dr. Sanderson is, however, careful not to endorse the conclusions which Dr. Bastian has drawn from these experiments." But this is scarcely a fair statement, since Dr. Sanderson had near the beginning of his letter announced his intention of taking no part in the controversy. Dr. Sanderson's opinions, however, on the elements of the question have already been set forth (see Thirteenth Report of Medical Officer of Privy Council, p. 59). Whilst not believing in the danger of atmospheric contamination by Bacteria germs, he does believe, in common with other biologists, that immersion in boiling liquids is a ready means of destroying them. If Dr. Sanderson had not thought that the conditions of the experiments were such as to be exclusive of the intervention of living germs, why should he have previously doubted "my statements of fact" in respect to them? Does Dr. Roberts consider Dr. Sanderson so much of a tyro in these matters as to suppose that he would doubt the well-known fact that living germs will always rapidly multiply in suitable fluids? If not, then the only other source of doubt that could have arisen, must have been as to the possibility of the appearance of swarms of living things in hermetically sealed vessels in which all pre-existing organisms had been killed. And if Dr. Roberts wishes ample proof that such has been the view also entertained by others I need only refer him to the last few pages of a curious article (purporting to be a review of my work "The Beginnings of Life"), which appeared in a recent number of a journal (*Quart. Journ. of Micros. Science*) of which Mr. Ray Lankester is one of the editors. It is perhaps fortunate for the reviewer's reputation and for his fame as a scientific experimenter that his name does not appear, or that his unsuccessful experiments, destined to upset my views, were not published before the advent of Dr. Sanderson's letter.

In some respects the actual results of the experiments performed by Dr. Roberts differ from those of other experimenters. Thus he has found that filtered infusions of any animal or vegetable substances can be "invariably preserved unchanged when boiled for five or ten minutes in a flask plugged with cotton wool." The results obtained by M. Victor Meunier and by myself have been different, and we have both shown that they are apt to vary according to the strength and nature of the infusions employed. Dr. Roberts says he has also found that many "highly putrescent mixtures" remained perfectly barren "after the flask containing them had been immersed in a water-bath kept at a boiling heat for twenty or thirty minutes," although several of the same mixtures "could not be kept unchanged by simple boiling over the flame," and the sealing of the flask during ebullition. If, after what I have already said concerning the latter mode of experimentation, anything is to be deduced from these facts, it would perhaps be that the partial vacuum within the flask is more favourable to the initiation of putrefactive changes in some boiled fluids than their contact with filtered air. This is what I have always thought, and evidence pointing that way may be found in *Appendix* C of my "Beginnings of Life." Certainly one cannot assent to the conclusion which Dr. Roberts would draw from such experiments, based upon the supposition that the boiling of the sealed flasks in water is a protective measure. Dr. Roberts' results are here again somewhat different from others which have long become matters of history. Need I say that this was essentially the method of experimentation introduced more than a century ago by Needham, and that his results were confirmed by his adversary, Spallanzani, who says: "L'ebullition d'une demi-heure ne fut pas un obstacle à la naissance des animalcules du dernier ordre qui peuplèrent toujours, plus ou moins, tous les vases exposés à son action pendant tout ce temps-là." Does Dr. Roberts forget that Dr. Wyman boiled his flasks for two hours and yet obtained positive results? that he boiled others in a Papin's digester under a pressure of two and five atmospheres respectively, and still obtained living organisms from his flasks. Must I also remind him of the numerous experiments by Prof. Cantoni, of Pavia, in which the hermetically sealed flasks were heated in a Papin's digester to temperatures ranging from 100°—117° C.; and to several of the experiments that I have myself recorded in which undoubtedly—living organisms were obtained from flasks that had been heated in fluids raised to temperatures varying from 130°—153° C. (*e.g.* such experiments as are recorded in "The Beginnings of Life," vol. i. pp. 441, 443, 447, and 463).

When will those who do me the honour of referring to my experiments look all round and cease to argue from one half of the facts? H. CHARLTON BASTIAN

University College, Feb. 24

Himalayan Ferns

DURING the years 1861–66 I took every available opportunity to collect ferns in the Sevalik and Himalaya ranges. There being at that period no published work on the ferns of British India, and one subsequently published in Madras not having come under my notice, my specimens, several hundred in number, and all well dried, remain unclassified.

Would this collection be of any scientific value, and if so, to what society could I present it? I opine it would be worse than useless to offer it to the herbarium of the British Museum, as there it might remain untouched for the next fifty years; whilst at Kew I presume Hooker's superior collection would render my poor mite useless.

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